the United States, of an importation of a real spiritualistic *mania*, far more injurious to our *mental* welfare, than that of the Colorado beetle will be to our *material* interests, I should be untrue to my own convictions of duty if I did not do what in me lies to prevent it. That I do not take an exaggerated view of the danger, will be obvious to any reader of Mr. Home's book. I know too well that I thus expose myself to severe obloquy, which (as I am not peculiarly thick-skinned) will be very unpleasant to myself, and unfortunately still more so to some who are nearly connected with me. But I am content to brave all, if I can believe that my *exposé* will be of the least service either to individuals or to society at large. W. B. CARPENTER

THE high scientific position which Prof. Foster holds, as well as the decided manner in which his letter was written, must lead the otherwise unbiassed reader to the conclusion that not only has a satisfactory explanation of the action in question been found and generally adopted, but that this explanation turns upon certain considerations, and particularly on the mean length of the path of the gaseous molecules as influenced by the degree of rarefaction.

I feel my position, therefore, particularly unfortunate in having, for the sake of truth, to show that the explanation which Prof. Foster has adopted, and supposes others to have adopted, is, if judged by the statements in his letter, inconsistent with well-established laws.

Prof. Foster gives me credit for having originated the fundamental idea of the explanation, but states that my "explanation was theoretically incomplete; in particular it did not show clearly why so high a degree of rarefaction should be necessary for the production of the phenomenon in question;" and then he proceeds to explain how this asserted deficiency was supplied by other thinkers, who showed that "the increase, resulting from rarefaction, in the mean length of the path of the gaseous molecules, would favour the action."

It is this supposed completion of my explanation that is erroneous. It is contrary to the law of the diffusion of heat in gases that "the increase, resulting from rarefaction, in the mean length of the path of the gaseous molecules would favour the action," and so far from supplying any deficiency in my explanation it is incompatible with it. The only result from such an increase is to diminish the action—a result which rises into importance only when the rarefaction is carried so far that the mean length of the path of a molecule becomes comparable with the dimensions of the inclosing vessel.

In my first paper I gave a definite proof, which has nowhere been questioned, that according to the kinetic theory the force arising from the communication of heat from a surface to adjacent gas of any particular kind depends only on one thing, the rate at which heat is communicated, and to this it is proportional. If therefore the increased rarefaction increased the force it must increase the rate at which heat is communicated, but according to the law established by Prof. Maxwell the rate at which heat is communicated is independent of the density of the gas, whence it follows that the increase in the mean length of the path of the gaseous molecules, resulting from rarefaction, cannot favour the action which remains approximately constant until the gas becomes so rare that the law of diffusion no longer holds, after which it may easily be shown the communication of heat, and hence the action in question, diminishes but never increases.

The fact that in the radiometer the force caused by the communication of heat only causes motion when the surrounding gas becomes extremely rare is, as I pointed out in my first papers, fully explained by the action of what I have called convection currents, which action depends on the weight and density of the gas. The gas adjacent to the hot surface is hotter than that which is more remote, and hence the former rises forming an ascending column, to supply which the gas is drawn in laterally on all sides, and tends to carry the surface forward with it. With the same difference of temperature and surrounding circumstances the speed of these convection currents is the same whatever may be the density of the gas, and hence the force which they exert on the surface is proportional to the density of the gas.

This force is opposite in direction to that arising from the communication of heat to the gas, and since the former diminishes with the density while the latter is constant, there must be some density for which they balance, and below which the constant force will predominate, while above this point the convection currents will carry the surface with them. The fact that, starting from low densities, the motion of the vanes in the radiometer does not only diminish as the density increases, but is actually reversed at higher densities, requires explanation, and no other than this has yet been offered.

I have gone into the subject at considerable length, as I felt bound, when venturing to differ from so high an authority as Prof. Foster, to state my reasons. There is, however, nothing in what I have said here which I have not said elsewhere, in the same or other words; and however incomplete in theory the explanation given in my first papers may be, I can only say that it included all the facts known to me at the time these were written; it has led me to predict many of the experimental results which have since been obtained, and I have not been able to find one fact with which it is not in accordance, nor has it been, so far as I am aware, controverted in any particular.

OSBORNE REYNOLDS

Potential Energy

I HAVE reason to believe that the "grievous error" with which I charged "John O'Toole" in his reference to the clock is not meant by him to be his own view of the matter at all, but merely a legitimate deduction from the confused and inconsistent language of "the doctors." Such an erroneous view on his part is, indeed, obviously out of harmony with the extensive knowledge of the subject of energy displayed by him in letters which, without doubt, will convince "the doctors" of the necessity of adopting consistent and strictly logical phraseology.

G. M. MINCHIN

Royal Indian Engineering College, Cooper's Hill

Effects of Urticating Organs of Millepora on the Tongue

AN article by Mr. Moseley, in NATURE (vol. xvi. p. 475), reminds me of an experiment I made some years ago in Florida. In collecting corals on the reefs, I had of course become familiar with the disagreeable, though not very painful, effects of contact of the hands with Millepora. But the vulgar names of Pepper-coral or Sea ginger induced me to try the effect on the tongue, to find out how far the taste resembled those condiments. I accordingly broke off a fresh piece and applied it to the tongue. Instantly a most severe pain shot, not only through that organ, but also through the jaws and teeth. The whole course of the dental nerves and their ramifications into every single tooth could be distinctly and painfully felt. I can compare the sensation to nothing better than to the application of the poles of a pretty strong galvanic battery. The pain remained severe for about half an hour, then became duller, leaving a sensation still perceptible five or six hours later. The whole impression was much too violent to allow the distinction of any particular taste.

Such an experiment made with Physalia might be positively dangerous, considering the much more powerful urticating effects of its polyps. Indeed, a friend of mine once related to me that when a boy he had come in contact with one of the long tentacles of a Physalia, when bathing, and had to be carried out of the water almost fainting. L. F. POURTALES

Cambridge, Mass., October 22

Drowned by a Devil Fish

THE following account of the destruction of a human being by a cuttle fish at Victoria, in Vancouver Island, has all the appearance of authenticity about it. It occurs in the *Weekly Oregonian* of October 6, 1877. The *Oregonian* is the principal paper of Oregon, and is published at Portland.

The insertion of the account in NATURE may lead to further information on the subject. I know of no other authentic instance of the kind.

An account of the habits of the huge octopus of the Vancouver Island Sounds and also of the Indian method of hunting and killing the beasts for food is to be found in John Keast Lord's "Naturalist in Vancouver Island and British Columbia," vol. i. p. 192. Mr. Lord measured specimens which had arms five feet in length, with a thickness at their base as great as his wrist, and he once collected a detached sucker of one of these cephalopods as large as an egg cup in mistake for a huge actinia.

"BRITISH COLUMBIA

"Drowned by a Devil Fish

"VICTORIA, September 27.—An Indian woman while bathing was pulled beneath the surface of the water by an octopus or devil fish and drowned. The body was discovered the following day in the bottom of the bay in the embrace of the monster. Indians dived down and with their knives severed the tentacles of the octopus and rescued the body. This is the first recorded instance of death from such a cause in this locality, but there have been several narrow escapes."

Exeter College, Oxford H. N. MOSELEY

The Earthworm in Relation to the Fertility of the Soil

IN NATURE, vol. xvii., p. 18, there is an account under the above heading of M. Hensen's investigations of this subject, to which I wish to add a note. He says the assertion that the earth-worms gnaw roots is not proved by any fact; roots gnawed by worms were never met with by him, and the contents of the intestines of the worms never included fresh pieces of plants. The experience of gardeners that the earth-worm injures pot plants may be based on the uncovering or mechanical tearing of the roots.

I should have thought that the universal experience of gardeners is that earth-worms never eat vegetable matter until it has decayed, and that their instinct leads them to draw the points of leaves as far as they can into their tubes for the purpose of setting up the decaying process, and likewise to sever the roots of pot plants with the same object. I can hardly understand how earth-worms have any mechanical means of severing the roots of plants except by gnawing.

But there is an omission in M. Hensen's account of the fertilisation of the subsoil by earth-worms which surprises me. He mentions but two ways in which this is effected, viz., by the opening of passages for the roots into deeper parts, and by the lining of these passages with humus.

I thought it was a well-known fact that worms, by means of their "casts," effect a complete *renversement* of the soil of meadow land down to a certain depth in the course of a few years. But whether well-known or not I met with a demonstration of this important fact in 1857. When putting down a considerable extent of iron fencing in the alluvial meadows near my house (consequent upon an exchange of land) I had occasion to cut a ditch two or three feet deep, and when the workmen had finished the ditch—a quarter of a mile long in all—I was astonished to see in one portion, of about sixty yards in length, a distinct and very even narrow line of coal-ashes mixed with small coal in the clean cut surface of the fine loam of the ditch face, perfectly parallel with the top sward. It immediately occurred to me that this was the work of the earth-worms, and upon inquiry I found that the farmer, who had occupied this land for many years, remembered having once, and only once, carted out some coal-ashes and spread it at this spot not many years before. I forget the exact number of years, but I believe it was about eighteeen. I have a distinct recollection, however, that the depth of the line of coal-ashes below the surface was at least seven inches, and that this seemed to confirm the general belief that the depth to which the earth-worm usually burrows is about that amount. I may add that the colour of the loam above the line of coal-ashes was decidedly darker than of that below.

HENRY COOPER KEY

Stretton Rectory, Hereford, November 2

IN NATURE, vol. xvii. p. 18, some details are given of observations made by M. Hensen on the relation of the earthworm to the fertility of the ground. He has observed, as everyone must have observed, that the earthworm during night draws into its tube or hole the loose leaves and fibres which may be lying about. But this operation of the earthworm has a significance in relation to the vegetable world of even a profounder kind than that of the fertilisation of the soil. Some months ago, in searching for young ash plants with three cotyledons, I found that in a great many cases the samara or seed of the ash had been drawn into a worm's hole, and had there found moisture and other essential conditions of growth; while the same seeds lying dry upon the surface had not germinated. There can thus be no doubt that many seeds of all kinds are drawn under the surface of the ground, or covered by the earth thrown up by worms. They are thus preserved from birds and various enemies, and are placed in the proper position for germination. The dead plant is perpetuated from its fallen panicle by the earthworm. An ash tree, or a whole forest of ash trees, may have been planted by earthworms.

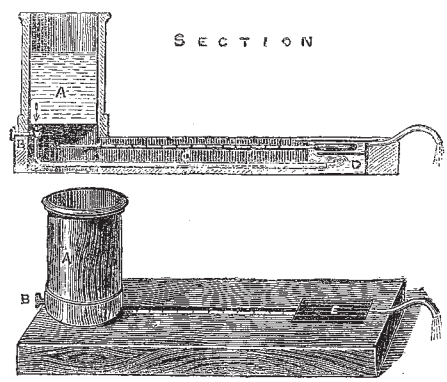
North Kinmundy, November 5 A. STEPHEN WILSON

M. Alluard's Condensing Hygrometer

THE notice of the above instrument in last week's NATURE (p. 14) is an excellent illustration of the necessity for increased communication between the scientific men of all countries. The labour which is at present wasted by repeating what has been done before is enormous, and until international intercommunication is improved it must be so.

I quite agree with you in your appreciation of M. Alluard's hygrometer, but I think it is desirable to state that it is not the first in which "the part on which the deposit of dew is to be observed is a plane well-polished face A, of silver or gilt brass." The annexed engravings represent the form of plane-faced hygrometer invented by Mr. G. Dines, F.M.S., described by him in the *Meteorological Magazine* for October, 1871, and exhibited at the Brighton Meeting of the British Association, 1872.

The action is extremely simple; no ether is required nor any aspirator. Water colder than the dew point is the only requisite—it is poured into the reservoir A, passes through the regulating-tap B into the chamber D; it is, by the black diaphragm, thrown past the bulb of the thermometer C, and then allowed to escape. The cooled plane surface E of silver or black glass, is excessively thin, and the space between it and the thermometer-bulb is wholly occupied by the effluent water, so that the great essential



of all hygrometers, a true indication of the temperature of the cooled surface, seems to be reached. The plate E can be kept within 0°·2 or 0°·3 for a length of time by adjusting the screw B, and as the condensation usually takes an elliptical form over the thermometer-bulb, and in the middle of E, the advantage of an adjacent bright surface is usually attained. I am, however, not sure that M. Alluard's surrounding plate might not be a convenience, although for the reason above given I have not found it necessary. G. J. SYMONS

62, Camden Square, N.W., November 2

Optical Spectroscopy of the Red End of the Solar Spectrum

NATURE, dated August 2 (vol. xvi. p. 264), containing Prof. Piazzi Smyth's communication on "Optical Spectroscopy of the Red End of the Solar Spectrum," reached me on the 21st ult., when I had no leisure to avail myself of the outgoing mail and reply immediately to the subject of his last paragraph. Inquiry is there made of "anyone" (besides the Royal Society), in association more or less with my name, whether *more recent particulars* have been published, of the spectrum in question, than "those (*i.e.* my) Indian observations," "printed in the *Philosophical Transactions* so long ago as 1874" (*i.e.* 1875).

2. The Astronomer-Royal for Scotland is presumably in a better position to reply for "any one," than myself, located in latitude N. 30°, longitude E. 78°; and so far as the inquiry relates to the Royal Society, his penultimate paragraph in itself furnishes the information sought, because the Society's publica-